Atmos. Chem. Phys. Discuss., 13, C236–C240, 2013
www.atmos-chem-phys-discuss.net/13/C236/2013/
© Author(s) 2013. This work is distributed under
the Creative Commons Attribute 3.0 License.

Interactive comment on "Long term changes in the upper stratospheric ozone at Syowa, Antarctica" *by* K. Miyagawa et al.

Anonymous Referee #3

Received and published: 25 February 2013

Review of "Long term changes in the upper stratospheric ozone at Syowa, Antarctica" by K. Miyagawa et al.

This paper presents an analysis of the long-term Umkehr record from Syowa. The authors show qualitative agreement with SBUV overpass data, and that the Umkehr record follows the EESC curve for an age of air of 5.5 years, or somewhat longer. It is not clear to me why they claim that "Ozone recovery during the austral spring over Syowa station appears to be slower than predicted". There is a great deal of statistical analysis attempting to identify dynamical parameters affecting ozone, but it is not very convincing, and the authors' conclusion that "dynamical and other chemical changes in the atmosphere are delaying the recovery over this station" does not really seem to be supported by any clear evidence.

The presentation in general is unclear, and seems unfocussed. There are too many figures, as some of them appear to merely illustrative and others are presented with almost no discussion. For example, Figure 2 presents no information that is not already in the text; the information in Figure 3a is also in Figure 4a; and Figure 11 apparently repeats (part of) Figure 7. Figure 9 doesn't present any information that is pertinent to the authors' conclusions.

I think the authors need to focus on the story they wish to tell, and drop some of the distracting side discussion. In some cases I think they need simply to omit discussion of some of the factors that they (commendably) examined; we don't need to know about every dynamical factor they looked at; just those that showed a statistically significant correlation with ozone. The paper should be rewritten, and made shorter. There is a little too much discussion of what other people have done, and not enough detailed discussion of what the authors did.

Detailed comments:

Page 385, lines 19-21: I believe this anticorrelation has more to do with the rate of the $O+O3$ loss reaction.

Page 387, lines 3-6: Apparently all 1508 profiles were usable?

Page 388: This is titled "Uncertainty for ozone profile retrieval", but doesn't actually discuss those uncertainties at all (not quantitatively), except to assure us that they have been "successfully corrected". Also, what are the special uncertainties of making Umkehr measurements at high latitude, where the sun takes a long time to change SZA and consequently makes a considerable change in azimuth as well? The authors note that few stations poleward of 60° even try.

Page 390, Equation (1): I think the terms are monthly averages. This should be stated.

Page 391, lines 20-21: This is surprising. The heat flux is presumably a dynamical proxy for meridional (ozone) transport. Since atmospheric motions are primarily zonal, wouldn't monthly zonal averages of the heat flux make more sense than coincident measurements?

Page 392, lines 3-4: That sounds like a good idea, but it doesn't appear to have been done. From my reading of the text, it seems that all the proxies were used, even those that made the fit worse (Table 2).

Table 2 is quite puzzling, and needs more explanation. In most of the summer cases, the minimum RMSD is achieved without all of the dynamical proxies, so that adding more proxies makes the fit worse! This should be a red flag that there is no information being added. Each proxy added adds one additional degree of freedom to the fit, so the fit should get better, even if the additional proxy represents random noise. The fact that it improves only very slightly in many cases with the addition of more proxies suggests that these carry no new information.

I am quite concerned as well about the time lags, which of course represent seven additional degrees of freedom to the fit. Most of the lags found are so long that it is difficult to imagine a physical mechanism responsible for them, as the authors admit. We are not told how much they improve the fit, and so one suspects that the improvement is actually coming from the additional degree of freedom added in each case.

The units for RMSD are not given, nor mean values, so it's hard to tell how good these fits are. I suggest adding correlation coefficients to the table as well. Are the numbers in (a) with or without the lags? How much difference do the lags make?

The authors state (p. 391, line 27 to page 392, line 4) that "Sequential addition of proxies ... allows for evaluation of independency of the chosen proxies for trend analysis. Correlation analysis of variability in different proxies also helps to eliminate non-orthogonal terms from the model". This is very laudable, but I don't see that this evaluation has been performed. If so, it is not discussed. All I can find is "After removing inter-annual variability in ozone data associated with the above parameters...".

Page 395, lines 18-20: "In summary, mean Umkehr ozone is lower than the homogenized SBUV ... and it shows good agreement with Umkehr. " This seems self-contradictory.

Page 396, lines 16-29: I don't follow this. Figure 4 seems rather to indicate that there are unresolved biases between the NOAA 17 and 18 platforms and previous ones. There are insignificant differences between the two curves in 4a. Also, in the figure caption, what is "ozone rate of overpass"?

Page 397, lines 1-5: This may be true, but why would that make ZM data better? Overpass data should show the same drift. More importantly, Hassler et al. conclude that the use of equivalent latitude corrects for this drift, and the current authors have used that as one of their dynamical proxies, so this should not be an issue.

Page 397, line 12: A correlation coefficient of 0.64 is really pretty awful, for measurements of the same thing! I would expect correlation coefficients of that magnitude for some of the dynamical proxies.

Page 397, lines 24-25: This statement is made without substantiation, and Figure 4 suggests that it is not true.

Lines 27-28: Some of these correlations are terrible: a number of those in Table 3 are even negative! What does this mean? How can they be so bad?

Page 398, lines 4-6: I don't see any ENSO correlation in the data presented. I believe this is wishful thinking.

Page 398, lines 10-11: Have we been shown any V8 data? Why is this statement here?

Page 398, lines 17-18: Not according to Table 2. Although that is what one would expect, since each proxy added adds one additional degree of freedom to the fit.

Page 400, line 27 - page 401, line 8: Why this discussion? 2006 doesn't appear especially low in Figure 6.

Pages 401-402: CUSUM analysis is not so well-known that it can be introduced with just a reference to Newchurch et al.. It should be explained. In any case, I'm not sure that this is an improvement over the straightforward method of showing that the data fit better to Polar 2.

Page 405, lines 10-17: I don't understand this argument.

Page 406, lines 1-4: Really? In what way did you "verify" this?

Page 407 lines 14-17: This difference is only 0.5%, and the losses are more than 20%.

Page 430: "Time series of monthly ozone variability..."? I think these are annual averages.

I would suggest dropping Figures 2, 3a, 9 and 11.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 379, 2013.